



This is the second of two installments of the Max Delbrück Oral History, excerpted from transcripts of half a dozen interviews conducted by Carolyn Kopp under a special program of the Caltech Archives. In the last issue of E&S, Delbrück told of his youth and his early career in pre-World War II Germany, where he was first interested in becoming an astronomer and then turned to theoretical physics. In 1932, however, he heard Niels Bohr's famous "Light and Life" lecture, a bold intellectual step by Bohr suggesting that aspects of quantum mechanics might have applications to other fields. That concept challenged Delbrück and, he says, eventually constituted his motivation to take up biology. Delbrück came to Caltech on a Rockefeller Fellowship in 1937, and when war broke out two years later found himself a de facto refugee in the United States. In the following chapter he discusses some aspects of his life and work in the past 40 years.

Max Delbrück

—How It Was

Max Delbrück: When I went to Berlin in 1932 — to the Kaiser Wilhelm Institute for Chemistry — my job was to be a theoretical physicist, as it were consultant, for Lise Meitner, a very good experimental physicist working on radioactive substances. I was supposed to keep up with the theoretical literature and watch out what happened, and also presumably be productive as a theoretical physicist and write theoretical physics papers. And I did write a few papers, not very interesting ones — a very *learned* paper with Gert Molière on statistical mechanics and quantum mechanics and an appendix to a paper by Meitner and H. Kösters on scattering of gamma rays. I never heard of the latter problem again until about 20 years later, in the fifties, when I was long since in biology. Somebody told me that there had been published two papers in *Physical Review* on "Delbrück scattering," by Hans Bethe and some graduate students of his who had made some progress in calculating the scattering. So since then this name "Delbrück scattering" exists, and if you ask theoretical physicists then I am known scurrilously for that little incident.

Now, I came to Berlin in the fall of 1932, and during the winter of 1932 and the spring of 1933 was the takeover of power by Hitler, and with it very quickly the beginning of the emigration of a large number of colleagues, especially Jewish colleagues, and the harassment of those who didn't leave; they either lost their jobs, or were not permitted to come to the institutes anymore, or to attend seminars. It was quite ridiculous.

Carolyn Kopp: How did you begin research in biology?

MD: After awhile there was a group of, as it were, exiled — internal exiled — theoretical physicists, five or six of us, who met fairly regularly, mostly at my mother's house, to have private theoretical physics seminars among ourselves; at my suggestion we soon brought in also some other people, some biologists and biochemists. They were Gert Molière, Werner Bloch, Ernst Lamlä, Werner Kofink, Kurt Wohl, Hans Gaffron, K. G. Zimmer, and of course, N. W. Timoféeff-Ressovsky, who was a staff member of the Kaiser Wilhelm Institute for Brain Research. We had Timoféeff over to my house a number of times, and we also went to his place just to see some flies, and talked about fly genetics and mutation research. His main line of research at that time was to study quantitatively the induction of mutations by ionizing radiations. In order to do this quantitatively, we had to have quantitative dosimetry of the ionizing radiation, and the person responsible for that was K. G. Zimmer. So out of that grew a rather lengthy paper, which summarized all the experimental data and methods, and then a big theoretical *Schmus* about interpreting it, for which I was mostly responsible. In a crude way one could say that the experimental results meshed together to the picture that the genes were relatively stable macromolecules.

The paper got a funeral first class. That means it was published in the *Nachrichten der gelehrten Gesellschaft der Wissenschaften* in Göttingen, which is read by absolutely nobody except when you send them a reprint. Timoféeff must have sent reprints around to all the major geneticists; when I came to Caltech two years later, A. H. Sturtevant, for instance, was

quite interested, although again, he didn't know enough physics. It was all a matter of bridging physics and genetics at that time — there just weren't any people who could do that. Sturtevant wanted to know what was in the paper, and so I gave a seminar here, and he was very pleased with that and said, "Now you have told us exactly what I wanted to know."

So this sort of black market research was going on, I mean it was moonlighting; I was supposed to be the theoretical physics adviser to Lise Meitner, but actually took all this time out to work in biophysics. During that time Otto Hahn and Meitner (who were great experts on radioactivity and the chemistry of radioactive substances for decades) followed up the discovery of Enrico Fermi that you could irradiate uranium with neutrons, and obtain quite a number of radioactive substances with apparently new chemical properties, which Fermi suspected to be transuraniums. Hahn and Meitner picked that up, and indeed discovered that when you irradiate uranium with neutrons, a large number of products arose which could be characterized by their half-lives and by the type of radiation that they gave off. These were interpreted to be elements 93, 94, 95, 96, 97, but very soon it became obvious that there were quite a few more than that, and so they were supposed to be isomers of the transuraniums.

I was very quick in interpreting all of these as isomers of these things, and in retrospect this was really immensely stupid of me; I should have guessed what was really going on, namely fission, but I, like everybody else, lacked imagination to see that.

CK: The theoretical physical problems never seemed to have really caught your wholehearted interest.

MD: Yes, that's true. Well, this wasn't really a theoretical physics problem; it was too trivial. It was something that any experimental physicist could easily have figured out. You didn't need any calculation; all you needed to know was that there was excess energy there; the neutron enters and there is enough energy there to blow the nucleus to pieces. You needed to just be able to add and subtract, and it just

didn't occur to anybody; and it didn't occur to anybody until they were literally forced to this conclusion only the year after I left. I left in 1937 and came here to Caltech and gave here a seminar in physics which then a few weeks later turned out to be everything wrong.

CK: How did your second Rockefeller Fellowship come about?

MD: One day I got a visit from a gentleman of the Paris office of the Rockefeller Foundation, who was just checking up on what former Rockefeller Fellows were doing. I told him what I was doing, and since I was reading this book on population genetics by R. A. Fisher, he suggested, "Don't you want to go to London and study with these people?" And I said, "Well, why not?" And then, however, after I reconsidered, I said, "I'm not really that interested. If I want to do something like a Rockefeller Fellowship I would rather go to Pasadena." And to my surprise he acceded to that without batting an eyelid, and to my surprise Hahn and Meitner — not to my surprise; I knew that I had their good will and friendship — they acceded to it and facilitated it by giving me a guarantee that I could come back and get my job back — that's what Rockefeller insisted on. And so the next thing was to get an exit visa to get permission to leave Germany. Before the Nazis, this problem would not have existed. There was no such thing as an exit visa, but at that time already I guess you needed some sort of an exit permit, because they had reinstituted military service. I was beyond the age of military service; in 1937 I was 31.

CK: You say in the Royal Society biographical questionnaire that one of the reasons that you wanted to go to the United States was because it seemed as if political factors would bar you from further advancement in Germany.

MD: Yes. While I was the assistant of Lise Meitner, I also tried to become a lecturer at the university; this means *Habilitation*, become a *Privatdozent* and obtain a *venia legendi*, permission to lecture, but unsalaried. The Nazis very quickly made



Photograph taken in 1934 for Nazi indoctrination camp.

this procedure more complicated by dividing it into two steps. One, you were supposed to get an advanced degree, as it were, the *Dr. habil.*; that means essentially presenting all the publications that you have made, demonstrating that you are scientifically, scholarly, qualified. In addition, you were, however, supposed to pass also some political tests. To do so you had to go to a *Dozentenakademie*, an indoctrination camp, which was quite a fascinating thing — a "free" discussion group, you know, where you got lectures on the new politics and the new state. So we had "free" discussions, and after three weeks of "free" discussions they decided whether you were sufficiently politically mature to become a lecturer at the university.

My first one, I think, the very first one that they had run themselves, was at a very nice estate near Kiel. There were about 30 of us, and in a way it was a marvelous thing, because it was the first time in my life I got thrown together closely with people from other disciplines. I learned more about other sciences at this academy and at the next one than anywhere else. But of course there was also the business of having these wonderful lectures by reliable party members, and everybody was terribly nervous because you really didn't know what was going on, and what you could say and couldn't say. Anyhow I obviously was too incautious, and I was informed afterwards that I wasn't quite mature enough but that I could try again.

So I tried again. The next time it was in another beautiful place, Thüringen. There things ran much more smoothly; everybody knew by then what he could say and couldn't say and everything was much more relaxed. But still I must have shot my mouth off. It must have been transparent that I wasn't in great love with the new regime, so I don't know whether I was officially informed that I wasn't mature enough, or whether they just didn't answer my letters. I have forgotten now. Anyhow it was pretty clear that a university career was not likely to be open to me.

So when this Rockefeller thing came around in 1937 it seemed like a good idea to see something of the world and see what was going to happen, because at that time it was anybody's guess how long the mess was going to last. Some people said six months and some people said much longer. I was immensely lucky that I had this opportunity. Many nasty things have been said about those who could have left and didn't leave, like Heisenberg, he's the most outstanding case. I don't agree at all with these derogatory comments. I don't think that it was anything to my credit that I left at all. I think it was a question which could be answered one way or the other, and there is great merit on both sides.

CK: It seems that the choices seem to be much more clear-cut in retrospect than perhaps they were at the time.

MD: Of course, yes. It's not that the choices seem clear-cut in retrospect, but they seem clear-cut to people who have no sense of the reality of the situation. I mean going away was in any case only a chance.

I went via England and visited a Faraday Society meeting in Manchester, I think, and then took a boat to New York. In New York I visited the Rockefeller Foundation offices and then spent a post-season month in Cold Spring Harbor. There I talked mostly to M. Demerec, and learned a little about work on *Drosophila* cytogenetics, using salivary gland chromosomes with their wonderful banding. Demerec also made me do a little experimental work, that is, actually dissect *Drosophila* larvae and fish out the salivary glands and squash them and stain them,

and that's as far as I ever got with *Drosophila* genetics experimentally.

After that month I went West and made only one stop on the way in Columbia, Missouri, to visit Louis Stadler. Stadler was sort of the counterpart to Timoféeff, in the sense that he (Stadler) had discovered the mutagenic activity of ultraviolet light; this is in contrast to the work of the other people who worked with ionizing radiations. Then from there I continued by train and must have arrived in Pasadena on the Santa Fe train one evening in late October. I was met at the train station by one German fellow, George H. M. Gottschewski, a *Drosophila* geneticist, and somebody else. They took me out for a beer, and dropped me at the Athenaeum, and Gottschewski got me all upset, because he said that Thomas Hunt Morgan was very upset about my coming; he didn't know what to do with this theoretical physicist, and really thought it was crazy for a physicist to come. Well, that turned out to be entirely wrong, but it was sufficiently unsettling for me, having traveled 8,000 miles to get here, that I from that day on was utterly confused about north and south in Pasadena.

The next morning, then, I visited Morgan, who was very cordial, and I explained that I had done these somewhat theoretical studies with Timoféeff — Timoféeff did the experiments and I did the theory on mutagenesis and ionizing radiation in *Drosophila* — and that I wanted to learn more about the actual *Drosophila* genetics, and see how the whole subject could be advanced further. Morgan suggested that I should work with A. H. Sturtevant. I talked to Sturtevant, who was also very nice, and he suggested that it would be interesting to try to clear up some confusing results on linkage in the fourth chromosome. He gave me some reprints to read, which I tried and failed to understand. By then the *Drosophila* terminology had become so specialized and esoteric that it would have taken me weeks even to understand all their terminology.

I sat poring over these papers pretty disconsolately for some time in the room across from Calvin Bridges, who was another very wonderful *Drosophila*

geneticist. So I consulted with him quite a bit and became very good friends with him. Calvin Bridges lived a "hippie" type of life — very simple. He had a small frame house here on one of the streets nearby, cooked for himself and occasionally had friends come in, but all very unobtrusive and very friendly. He and I regularly went for lunch together, which consisted of going to the corner of Lake and California and buying there in the market for 10 cents some peanuts and for 5 cents a little bottle of milk, and then we walked back and sat on the bench at the bus stop, and consumed our peanuts and milk and chatted about everything, both science and many other human things. In the Old World I had never met a person so unpretentious in a way that only an American can be unpretentious, although he was a really outstanding scientist. He died a year later.

I consulted with him for quite a bit and tried to learn some *Drosophila* genetics and, as I say, I didn't make much progress in reading these forbidding-looking papers; every genotype was about a mile long, terrible, and I just didn't get any grasp of it. So then one day I read that a seminar on bacteriophage had been given by E. L. Ellis, while I was away on a camping trip with Frits Went, the plant physiologist. I was unhappy that I had missed it and went down to ask him afterwards what it was all about. I had vaguely heard about viruses and bacteriophages, and I had read the paper by Wendell M. Stanley on the crystallization of the tobacco mosaic virus before I had left Germany. I had sort of the vaguest of notions that viruses might be an interesting experimental object for a study of reproduction at a basic level.

Well, Ellis was very cordial and showed me what he had accomplished by then, which was really very impressive; starting from zero knowledge concerned with anything about microbiology, viruses, and so on, he had gotten together very primitive kinds of equipment — an autoclave and a sterilizing oven, a few dozen pipettes, a few dozen petri plates, and some agar — and had taught himself how to pour plates and to use sterile technique. He had gone down to see his

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friend, Carl Lindegren at USC, who was in the bacteriology department, and had gotten from him this strange organism that nobody had heard of before, called *E. coli*, which is now the thing that you hear about in grade school. And he had gone to the Los Angeles Sewage Department and gotten himself a liter of Los Angeles sewage, and from this sewage had isolated a phage active against *E. coli*. With that he had taught himself how to get plates that would produce nice plaques of the phage, and had, in essence, already shown something like a one-step growth curve. I don't know really how far he had gotten with that.

Anyhow I was absolutely overwhelmed that there were such very simple procedures with which you could visualize individual virus particles; I mean you could put them on a plate with a lawn of bacteria, and the next morning every virus particle would have eaten a macroscopic one-millimeter hole in the lawn. You could hold up the plate and count the plaques. This seemed to me just beyond my wildest dreams of doing simple experiments on something like atoms in biology, and I asked him whether I could join him in his work, and he was very kind and indeed invited me to do so. And so I did, after asking some other people like Bridges and Frits Went whether they thought this was a good idea. They encouraged me, so I dropped *Drosophila* and teamed up with Ellis. And that was just marvelous. We had a tremendous time; a tremendous time because it was all really new, at least to us and certainly to everybody in this building (Kerckhoff Labs), and pretty soon we also did a few things that were not generally known.

A few weeks or months afterwards Ellis gave a seminar on phage, and he brought some petri plates along to show these plaques; these were passed around and everybody said, "Ah!" A few days later I met Mrs. Morgan, who also did work in genetics, and I asked her whether she was impressed with these plaques. She said, "You know, the light was very poor. I couldn't see them." It turned out that nobody had been able to see them. Everybody had taken it on faith that there were plaques there, which I thought was quite

hilarious. It reminded me of the story of the emperor's new clothes.

Ellis and I worked together for a year, and after a year, unfortunately and to my great regret, Ellis dropped out of the phage thing, and went back to what he had done before — cancer research on transplantable tumors in mice. Apparently the fellowship under which he worked stipulated that it should be on cancer research. But he came into the lab and certainly continued to take an interest in what I was doing my second year here.

CK: Did you have any trouble renewing your Rockefeller Fellowship for another year?

MD: Not really. No, that was relatively simple. I came in the fall of 1937, and it was renewed to start in September 1938. This ran out after the war had started, which made it virtually impossible for me to go back to Germany; not that I was keen on going back, but it also left me high and dry without visible means of subsistence. For several months I lived on money borrowed from friends.

CK: There was no possibility of a position at Caltech?

MD: There might have been, but Morgan didn't come forward. He thought maybe, that wouldn't have been a healthy thing to do; although I'm sure he had a high regard for me — this was not the way he handled things. However, then the Rockefeller Foundation itself took a mild interest, and drew my attention to this job at Vanderbilt. In fact, an arrangement was made by which the Rockefeller Foundation paid half of my salary — the full salary was \$2,500 a year — in return for a gentleman's agreement that I would have half time free for research and would not be just loaded down with teaching physics. So a few days after Christmas of 1939, I left Pasadena and drove East, and arrived in Nashville on New Year's Eve in a driving snowstorm.

I got myself again set up at Vanderbilt in biology. I used the incubator and the sterilizing facilities of the department of bacteriology, which was a one-man department. My room was sort of in a no



Cold Spring Harbor, 1955. The barber is Seymour Benzer, now James G. Boswell Professor of Neuroscience at Caltech.

man's land on that floor between the physiology department and the bacteriology department. I may have gotten my own equipment after a while. I diddled along there, and then, I don't know in what sequence, I was joined by other people.

CK: You met Salvador Luria in December of 1940.

MD: And he did not come to Nashville until nine months later. I don't know whether by then I had some other people working there. Some of the earliest were A. H. Doermann, who had just gotten his degree in *Neurospora* genetics with George Beadle at Stanford; and E. S. Anderson; and gradually we took up contact with A. D. Hershey, who was at that time in the microbiology department of the Medical School at St. Louis.

And Tom Anderson, the electron microscopist; we first contacted him one summer when he was in charge of using the RCA electron microscope at Woods Hole. He had an exhibit instrument there, and collaborated with anybody who wanted to use it. He and Luria had already started in the summer of 1942 working on phage, and I joined them also for a few weeks.



Cold Spring Harbor, 1953. With Delbrück is Salvador E. Luria, with whom he shared the Nobel Prize in 1969.

Actually, it turned out that the findings we made that summer had been made previously by H. Ruska in Germany, but during the war there was very little communication. So the fact that some of these phages had this very odd shape, with a head and tail, and very startling morphology, had been seen in the electron microscope by Ruska, and had been published in the *Naturwissenschaften*. We did it a little more quantitatively, since we paid great attention to controlling two things quantitatively; that is, really control the concentrations of bacteria and phage, and the time in which they interact, so we could be a little more precise as to the adsorption process.

CK: Could we talk about how the first course at Cold Spring Harbor was set up in 1945, when you got the idea for that?

MD: I don't remember who suggested it, but that must have been already the fourth summer then; the first summer that we did phage work in Cold Spring Harbor was

1941, and I think from then on we were there every summer. So in 1945 then we gave this first course, which had a marvelously motley crew of students.

CK: Would you say that there was a sense that you needed to convert people to join in the research?

MD: You mean why did we give this course? I think Luria was the promoter of that. Luria thought that if phage ever was to become an important line of research, and its potentialities really developed, more people would have to be brought into it. And therefore one should make an effort to bring more people into it this way, by giving the course. Anyhow, it helped, even though only a few of the people who took the course actually became phage workers. At least this way we recruited quite a number of people who could read the phage literature with understanding.

CK: When did you hear about O. T. Avery's isolation of DNA as the "transforming principle" in pneumococci bacteria?

MD: Avery made his great discovery in 1943, but we knew about his working on this problem for at least a couple of years before then, and I think both Luria and I had gone to visit with him. And also Demerec knew quite well that there was a very interesting problem. It had been shown that you could use an extract of one bacterium and expose another bacterial strain to it, and then get some kind of transformation, and the transformation was expressed in producing a particular capsular polysaccharide.

The feeling had been that the transforming agent was the polysaccharide itself, that somehow that was sort of a crystallization process, or rather, a nucleation process; you add a piece of this polysaccharide and then more is produced; that was the obvious interpretation at the time. If that was true, then it showed that here you had a genetic property which was not transmitted by genes, but by something more like a whole organism, you might say; like every little piece of polysaccharide was a little apple tree that could grow

into a big apple tree. However, this little apple tree did not contain genes but was just a form principle that had made it possible to accrete more in the same form — more like a crystallization process. If you dump into a saturated solution a crystal of a particular substance, then you can get more of that crystal; it's a nucleation process. And if that had been true, it would not have been so overwhelmingly interesting, because it was obvious that this could not be the general principle of genetics. So it came as a total shock and surprise when Avery and his associates discovered that the transforming principle was DNA. He communicated this discovery to his brother Roy Avery at Vanderbilt, who was in the department of microbiology in the Medical School, in a 17-page handwritten letter, which Roy Avery showed me just about the day he received it, and which I read there standing in his office in the spring sunshine, I think it was. It was quite an amazing letter and has been published.

This discovery, of course, was just the beginning of the battle, because immediately the scientific world split into those who believed that their experiments showed that DNA is an information storage molecule, and those who believed that the DNA preps were contaminated with a small amount of protein, that the protein was the important part. During the subsequent years it was essentially the work of Rollin D. Hotchkiss who gradually tightened the proof more and more to show that the DNA is the essential thing.

CK: I am curious as to whether, when the Watson-Crick structure of DNA came out, there was a general feeling among biologists that this really marked a revolutionary point in biology.

MD: Let's put this question into two questions: whether I thought so and whether there was a general feeling.

CK: I know you thought so. You wrote to Bohr that you thought it equaled the Rutherford discovery of the nucleus of the atom. So you still think so in retrospect?

MD: Oh sure. Easily. The other half of the question — I think there was consider-

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able hesitation as to whether the structure was true. Shortly afterwards there was a Cold Spring Harbor symposium, and some of the more knowledgeable chemists were quite doubtful, (a) whether it's true, and (b) whether it would ever be possible to prove that it was true.

Now it's an interesting fact that there are several aspects to the structure, and people have become aware that the alternative models that have been proposed cannot be dismissed out of hand, that this double-helicity has never been adequately proved. Well, then the next question was, granted that the model is true, is the replication occurring in the way the model suggests; namely, each strand making its complementary strand. And that immediately poses a problem as to how the two daughter double helices are taken apart, how their knots are resolved. And that problem is still unresolved, or incompletely resolved.

Another question was, what do you do with this information that is stored there in the DNA? How do you go from there to really making proteins? And that has been largely resolved in the sense that we know how the amino acid sequences in the proteins are coded for a template code, but here again in the last couple of years it has been found that in eukaryotes, all kinds of monkey business occurs; that the gene that codes for a certain messenger RNA — which then is translated into protein — that this gene contains interstitial pieces that are eliminated later, and the meaning of that nobody knows yet. So there are still surprises.

CK: To back up a bit: When Erwin Schrödinger's book *What Is Life?* was published in 1945, what was your reaction to it? Had you known that he had discussed the model of the gene that you had put forward 10 years earlier, in the paper with Timoféeff and Zimmer?

MD: No, it was a total surprise to me. I had not seen or heard anything from Schrödinger, or by Schrödinger, for years, and when the book came out it was other people who drew my attention to it. I was puzzled how he had gotten hold of the paper, which he obviously had read, and which then formed a central chapter in the



The award of the Nobel Prize in 1969 led to this press conference in Millikan Board Room and to worldwide recognition of Delbrück's pioneering studies of viral genetics — the way strains of the virus bacteriophage infect the bacterium *E. coli* and multiply there.

book. I have recently learned, I think from the historian of science Robert Olby or somebody else, that it was not I who had sent a copy of the paper to Schrödinger, but that P. P. Ewald had shown him a copy.

CK: Did that book have the effect of increasing people's interest in what you were doing then in 1945?

MD: Insofar as it was read by a large number of younger, and not so young, people and physicists, it was publicity for me, although not specifically publicity for phage, more for genetics and for the problems posed by genetics. I mean I didn't need publicity, I would say, but maybe I owe my job at Caltech to it, I don't know. I doubt that I did, because Beadle knew me personally quite well when he offered me the job, and also the people here in the division had seen me around for two years; I don't think they needed Schrödinger's book when the question came up whether they should offer me a job here, which was done in December 1946, and the book came out about a year earlier. But I don't know what went on here.

CK: You were happy at Vanderbilt but were quite sure that you wanted to move?

MD: When the question really came up, to stay or not to stay at Vanderbilt — I mean when the people at Vanderbilt realized that I was very much in demand after the war, and then I got offers from Illinois, and Cold Spring Harbor, and from here, and from Manchester, England — then all of a sudden they tried to really promise me anything, and I think I was quite willing to listen, but I think Manny, my wife, was not, as I recall. In any case,

when the offer from Caltech came, it was irresistible.

CK: You came to Caltech in 1947 and were Beadle's first faculty appointee in biology. I was wondering what changes you've observed in the biology division since 1947.

MD: Well, it got bigger, which is not necessarily fortunate, and its emphasis shifted to chemical biology when Beadle came, more to molecular biology at first; then very soon the psychobiology was added — the Roger Sperry group — and that was an interesting move. This was made possible by a fund that Caltech had received, the so-called Hixon Fund, which was obtained for research that would do something about juvenile delinquency. From year to year the Hixon Committee struggled to find something that could be interpreted as having even the remotest connection to juvenile delinquency, and at the same time be compatible with the general attitude at Caltech of doing basic research. After having struggled for a number of years with that — arranging conferences, having visiting professors, and so on — the committee disembarassed itself by appointing Sperry as the Hixon Professor, so from then on *he* had to worry about how to reconcile this. (I was a member of that committee.) That was an important move, and the contributions of Sperry have been enormous.

CK: Do you think the division has made an effort to identify new and coming fields?

MD: Well, they considered bringing me here as being a new and coming field, and in recent years certainly they have in



In 1978 Delbrück was speaker for Caltech's commencement, an occasion that gave his colleagues a welcome opportunity to honor him for his many contributions to science and to the Caltech community. *Maximus est*, translated from the Latin, testifies that he is the greatest.

eukaryotic molecular genetics made several important appointments.

Then there was, of course, a period where Caltech went into animal viruses quite strongly. That was initiated in 1950 and, similarly to the Hixon business, came about through a stimulus from the outside — namely, a wealthy citizen who suffered from *Herpes zoster* was persuaded to offer Caltech \$100,000 to start working on animal viruses.

CK: After the war you returned to Germany several times.

MD: My first visit back to Germany after the war was in 1947 when things were still very chaotic, very chaotic.

CK: What was the psychological state of the scientists that you met at that point? Was there much guilt among the scientists you met who had stayed?

MD: It depended on who. No, if anybody feels guilty, I feel guilty of *not* having stayed, because I have so many friends who I admire for having stayed, and having tried to save what was to save, rescue it across this disaster.

So this was 1947, and then I must have visited a number of times afterward, but the first time I came for longer was in 1954. Then I came for three months and went to Göttingen. I came back for three months in 1956, to Cologne as a guest of Josef Straub, who was a professor of botany and who wanted me to bring molecular genetics to the university there. At that time his institute was still in a bunker in the Botanical Gardens, sort of subterranean caves. At that time the first new university institutes were being built, his among them. In fact, I think I gave a phage course there in this new building.

They had no electric light yet, and no cement floors, but yet we moved in and gave a course there, which was quite a tour de force.

At the end of this stay, they wanted to offer me a job, and I just couldn't see myself moving from Pasadena to Cologne. In the end I made a mistake. Straub said always, "Name your conditions." So the last day I was there I named conditions which I hoped would be so astronomical that the matter would end there. But then due to the fantastic negotiating ability of Straub, the thing finally became a reality in 1961, and we went there from 1961 to 1963.

At that time already all over Europe there were new universities being founded, and similarly in Germany they created a number of new universities. By hook or by crook they involved me in the founding committee of one of them — in Constance — as a consultant for the natural sciences faculty. This led to a natural sciences faculty that was essentially all molecular biology — even the chemistry and the physical chemistry were all molecular biology. We went there at an early stage for the summer semester of 1969. That was my last long stay in Germany.

The German universities have had their revolution like the rest of the world's universities, but I haven't seen much of it. The Max Planck Institutes — the renamed Kaiser Wilhelm Institutes — have expanded enormously; I think they now have 80 institutes, some of them quite monster places — huge places, and I don't think they are as productive as they should be. On the whole I have a feeling that nobody there really knows whither research and education are going to move.

CK: Because things have just been getting larger and larger, and there must be a breaking point, or why?

MD: For reasons as explained in my commencement speech (*E&S*, September-October 1978). The pristine faith in science has been punctured, and it's obvious that science is not going to solve our problems. Science is just as much a destabilizing force as it is a stabilizing force in the world. That's a very general thing. Specifically in Germany it's weighted down with all these problems of institutional lethargy and vested interests that go with it.

CK: Do you find now that — as expressed in your commencement address — you really have strong doubts about pursuing science the way it has been pursued in this country and other countries for the last 20 years?

MD: Yes, the honeymoon is over.

CK: You mean, it's over in that there seems to be a sense that science does not solve all our problems, and there is also a distrust of science by the public?

MD: Even by the scientists. I guess one would like to know more where really our values come from. And so you can ask where do the values come from, and you can ask what should our values be, and if you have an answer to what our values *should* be, how do we get them to be our values. These are not questions of science, but they are the questions, the answer to which will decide the further course of history more than anything else. I think the further course of history will not be decided by further discoveries in science, but by these questions about human values.

CK: Do you think it's possible that science will continue but that scientists will become more involved in value questions?

MD: No, I think the scientist, insofar as he is a scientist, has to do what he did before. Scientific *institutions*, like Caltech, will have to become more involved in value questions. □